and working at the same speed, the motion to the one hand will produce *red*, and when reversed, *blue?* Mr. Hannay also seems to imply that the colours of my experiments to be seen well, should be looked at passively or without keen attention. On the contrary, the more light thrown on them, and the keener, fresher, and younger the eyes of the observer, the more brilliant are the colours, and if a boy of ten or twelve years old, who never saw anything of the sort before, be called in, he will describe them better than grown people.

Scientific men have hitherto considered it a sufficient explanation of these experiments to say the effects are "physiological," as if colour were ever anything else. Newton says, speaking of *coloured rays*, that he uses the term merely to suit the understanding of the vulgar, as they are nothing but a certain power and disposition to stir up a sensation of this or that colour. Prof. Ogden Rood again classes these as *subjective* colours, a word which, if it has any meaning at all beyond a very limited one, can have none with reference to colours which remain permanent so long as the machine is in motion.

27, York Place, Manchester, April 22 NAPIER SMITH

How may Clouds consisting of Liquid or of Frozen Water be Distinguished?

IN NATURE, vol. xxv. p. 529, M. de Fonvielle asks my opinion as to what observations may be made in a balloon to discover whether in a cloud whose temperature is below zero the minute particles of water are liquid or solid.

There may be difficulties in the way of deciding by direct observation of the form of the particles, whether they are globules or crystals. But since H. B. de Saussure, a century ago ("Essai sur l'Hygrométrie), by means of a single lens, could distinguish in the air over heated water, globules of condensed water of different size, greater ones which appeared to him full, and smaller ones which he regarded as hollow; and when in more recent times A. Waller (*Philosoph. Transactions*, 1847) could make his "microscopic observations on the so-called vesicular vapour of water as existing in the vapours of steam and in clouds, &c.," with the result that he denied the existence of mist-vesicles, it seems possible that by means of a magnifying instrument the form of the particles suspended in the air can be recognised.

A sure evidence, but obtainable only under favourable circumstances, that the particles forming a cloud are ice-crystals would be the observation of the halos of 22° and 46° radius and of parahelions as produced by the cloud. HERMANN KOPP

Heidelberg, May 3

On the Conservation of Solar Energy

Dr. SIEMENS's theory of the constitution of the sun implies that there is an absorption of solar rays constantly going on in space. If this is so, space cannot be perfectly transparent.

An astronomer of the early part of the present century—I think it was Olbers—came to the same conclusion, though from different reasons. He found that as the space-penetrating powers of the telescope is increased, the number of stars that become visible does not increase so rapidly as it would if they were evenly scattered through space, and if space were perfectly transparent; and he concluded that most probably space is not perfectly transparent. This, however, is by no means conclusive, because it is possible that the reason why the number of stars that become visible does not increase as it ought to do on the supposition, is that the number of stars in the universe is limited.

JOSEPH JOHN MURPHY

Old Forge Dunmurry, co. Antrim, May 3

CYCLONES[1]

II.

IN our former article we dwelt on the deductions arrived at by the author from a consideration of the mechanical theory of cyclones. We will now proceed to examine how far such theoretical relations are corroborated by the results of observation. The results of observation utilised by the author comprise those of the Rev. W Clement Ley, published in his "Laws of the Winds"; those of Prof. Loomis, deduced from a study of the U.S. Signal Service charts; those of Dr. Hildebrandsson, with regard to the upper currents from an examination of the Danish synoptic charts; those of Capt. Toynbee, from a study of the Atlantic storms; and lastly, some contained in a recent work on the hurricanes of the Antilles, by Padre Viñes of Habana. Mr. Ferrel at the outset pointedly remarks that for a mariner to be able to make use of the laws deduced from a study of the theory of cyclones, not only a knowledge of such laws is requisite, "but likewise of the *normal* states of the wind and of the barometric pressure in all parts of the ocean and at all seasons of the year, unaffected by the abnormal disturbances of these progressive cyclones; since with a knowledge of the normal conditions of the winds and of the barometric pressure at any time and place he can perceive the first indications of the abnormal disturbances which are the forerunners of these storms, and so can be on his guard, and then with a knowledge of these storms or cyclones, he can generally avoid at least their most dangerous part."

With regard to the first result from theory, viz. the general incurvature of the winds in a cyclone, which was formerly altogether denied by the cyclonists—so-called—Reid and Piddington (not Redfield), or inordinately magnified in every case by Espy, and other upholders of the radial theory, there seems to be no doubt from the results of observation here given, as well as from others not cited by the author, that the wind deviates to a considerable extent from the tangent to the isobars inwards towards the low centre. Moreover, in accordance with theory, this inclination is greater at inland stations where there is more surface friction than at or near the sea when it is less. Thus Ley found the inclination to be about 29° for inland stations, but only 13° for those on the coast. This difference between the inclination at sea and on land may perhaps account for the tenacity with which sea captains still cling to the notion that the wind blows in circles coincident with the isobars; since it is precisely at sea where the incurvature should theoretically be least. The increase in the inclination corresponding to a decrease in the latitude is likewise borne out by observation. Thus from Ley's observations, which embrace North and South Europe, the mean inclination to the isobar is about 25°, from those of Capt. Toynbee on latitude 50° it is 29°, from those of Loomis nearer the equator in America 47°,[1] from those of Padre Viñes in the Antilles 45°, and we may add from some in the Bay of Bengal, mentioned by Blanford, about 42°.

It is thus pretty evident, as the author remarks, that "though the horn-cards of Piddington based on the strictly circular theory of the winds may still be used at sea in high latitudes without great error, yet nearer the equator they must become more erroneous, and entirely fail at the equator if cyclones could exist there." Mr. Meldrum, of Mauritius, as far back as 1867 drew attention to the disasters which resulted in consequence of vessels having estimated the direction of the centres of cyclones according to the rules of the circular theory, and since the publication by M. Faye of his "Défense de la loi de tempêtes," has strenuously opposed the resuscitation of this exploded doctrine. He has also lately given an admirable proof of the truth of the incurvature, by publicly announcing when the wind, in a cyclone on March 21, 1878, was from the north-east in Mauritius, that the centre of the storm was not to the north-westward according to the circular theory, but to the west-south-westward, which was afterwards found to have been the case.

It is a manifest duty therefore which mankind owes itself, if the dangers of the sea are to be minimised, that the amount of inclination of the wind to the isobar should be determined by observation in different seas and for

[1] "Methods and Results of Meteorological Researches for the use of the Coast Pilot." Part II.—On Cyclones, Waterspouts, and Tornadoes. By William Ferrel. (Washington, 1880.) Continued from p. 12.

[1] Some of these included very gentle winds.

different latitudes, in order that the navigator may be able to modify his rules accordingly, and so avoid such a fatal error as that of running gradually into the centre of a storm, which a rigid adherence to Piddington's rules would be certain to entail.

So far we have not considered what effect is produced on the inclination by the progressive motion of a cyclone. Were a cyclone regular in form and stationary, the inclination should obviously be the same at every point on the surrounding isobars. When, as is generally the case, it has a progressive motion, this—on the supposition that it is mainly due to the general motions of the atmosphere—should, by a simple application of the parallelogram of velocities, alter not only the velocity but the direction of the inflowing winds, increasing their inclination in the rear, and diminishing it in the front part of the cyclone.

This conclusion tallies remarkably well with the observations of Padre Viñes in the Antilles, where the cyclones travel westwards, as well as with those of Prof. Loomis in North America, where they travel towards the east. When we come to Europe, however, a remarkable exception to this rule occurs, since here the inclination is much *greater* on the east or front side (especially south-east) of a cyclone than on the west or rear side.[1] The author attempts an explanation of this fact on p. 40, but a better one both of this and of the equally enormous though opposite difference found by Loomis between the inclination in the rear and front parts of cyclones in the United States, which can hardly be altogether due to the admittedly small velocity of the general motion of the air over America from west to east is given by Lieut. Spindler, of the Russian Navy, in a recent number of the *Repertorium*,[2] where he considers it to be mostly due to the friction encountered by the wind on the Continental side of cyclones, increasing the inclination in the west and therefore rear part of the American cyclones, and that in the east or front part of the European cyclones.

If the general truth of the difference between the inclination in the front and rear of cyclones at sea, due to their progressive motion, be admitted, the case of the poor navigator becomes still more complicated, as in addition to considering latitude, distance from the centre, and velocity of the wind, he must likewise consider in what quadrant of the cyclone he is situated, since the direction of the vortex with reference to that of the wind, is so different in different quadrants. Fortunately it is just in front of a cyclone—the most dangerous position for a ship to be in—where the old circular rules are least at fault, since it is precisely here where theoretically the inclination should be least.

With respect to upper currents, the results derived from theory are remarkably confirmed by those from observation. In general, since the air flows in towards the centre below, it must flow out from it above, and also somewhat across the current which flows below, so that if we stand facing the wind at the earth's surface, and at no very great distance from the low centre, the upper current should almost invariably flow from some point to our right. This agrees with observation, since, according to Clement Ley, the average direction of the upper currents is 44° to the right of the direction from which the surface-winds blow. The general conclusion arrived at by Mr. Ley regarding upper currents, that they "manifest a centrifugal tendency over areas of low pressure, and a centripetal over those of high," is identical with that arrived at by Dr. Hildebrandsson, and with the author's theory as far as it applies to ordinary, or as he calls them, warm-centred cyclones.

Cold-centred cyclones, to which we alluded in our previous article, do not seem to have been identified by the author except in a stationary form surrounding either pole. The observations of the upper currents hitherto made indeed argue powerfully against their existence at all in the progressive form, since in their case the upper currents should flow in towards the low centre, accompanied by a gentle outflow below, a state of things diametrically opposed to all present experience in connection with a central area of low barometer.

The author next discusses the effect of the general progressive motion of the atmosphere on the upper currents, which is similar to that on the lower currents, but larger in consequence of its increase with the altitude.[1] In winter when the progressive motion is theoretically larger than in summer (Part I.), the upper currents in our region should in nearly all cases move from some westerly point, acquiring their greatest velocity on the south side of the low centre. In summer the directions should be more variable, and the wind's velocity less. Observation verifies both these conclusions; thus Clement Ley found the greatest velocities of the upper currents, such as 120 miles an hour, to occur generally in the winter, when the cyclone centre was to the north or north-east, and it was travelling eastwards. On the other hand the cases in which the upper clouds were found to be stationary most commonly occurred in summer, and near the centre of areas of high pressure. These facts, both as regards the strength and direction of the upper currents, are confirmed by Prof. Loomis's observations of the winds on Mount Washington. The author then proceeds to show how the upper currents may be employed to indicate through the medium of their visible accompaniments—the cirrus clouds—the approach and direction of a distant cyclone; a point most valuable to the seaman, who cannot command a daily weather chart. He says: "The almost universal precursor of a distant storm is the appearance of more cirrus-clouds than usual, not only differing from those of the general currents in form, but also in the *direction* of the currents indicated by those clouds."

In low latitudes, where according to theory the upper and lower currents are more nearly radial to and from the centre respectively, the direction from which these clouds come, especially while the storm is still at some considerable distance, is found to indicate very nearly the direction of its vortex. In higher latitudes matters are more complicated, since the currents are more tangential, the upper currents flowing anticyclonically at great distances from the centre, but even here, the observer will not, as a rule, be far wrong according to the author's diagrams, and those of Clement Ley, if he places the centre of the approaching storm a little to the right of the direction from which the cirrus advances.

Regarding the existence of an anti-cyclone in connection with every cyclone, and the broad annulus of high barometer with its maximum at the dividing limit between them, the author finds ample confirmatory evidence; though from the fact that the depression at the centre is more marked than the rise of pressure near the border of the cyclone, the latter is often so masked by other irregularities as not to be readily discernible on a synoptic chart. In our own islands, where we frequently encounter a string of small cyclones travelling over us from the Atlantic, the barometer rises briskly after the passage of each low centre only to warn us of the approach of its successor, from whence no doubt arose the old maxim, "Quick rise after low foretells stronger blow." The author mentions that the approach of the hurricane of September, 1875, was indicated at Havana by a sudden rise of the barometer,

[1] In the north-west quadrant the inclination according to Ley is only 9°, while in the south-east quadrant it is 35°.

[2] Ueber die Abhängigkeit der Stärke und Richtung des Windes von der Grösse und Richtung des Gradienten an den Küsten des Baltischen Meeres. "Rep. für Met, tom. vii. No 5, St. Petersburg, 1880.

[1] This increase in the velocity of the general atmospheric drift with the altitude is shown in Part I. p. 45, to result from the relations between the velocity of the wind and the observed barometric pressures and temperatures in different parts of the world. For the latitude of the British Isles the eastward component of velocity at the elevation of five miles—the height of the cirrus-clouds—is estimated to average about sixty-three miles per hour in January, twenty-nine miles per hour in July, and throughout the year about fifty miles per hour. At the surface, the mean velocity is calculated to be four miles per hour in January, and two and a half miles in July.

while the cyclone was yet at the Windward Islands, about 1200 miles distant.

A sudden and abnormal rise of the barometer thus constitutes as important a warning to a navigator as a similar fall, or a band of cirrus-cloud, only to be able to make effective use of this danger-signal he ought accurately to know the normal height of the barometer where he is, and at the time of year. The author shows how this may be done in Part I., where he has constructed charts, based on a large series of observations in the northern hemisphere, showing not only the curves of mean annual pressure, but also those which represent the coefficient of annual inequality. From a simple equation involving these two elements, the normal pressure at any time and place can be approximately reckoned, and hence the amount of abnormality determined.

The author next applies the cyclone theory in explanation of the various inequalities of barometric pressure, which are observed on the same latitude in different longitudes. These inequalities he considers to be mainly dependent on the deviations of the mean temperature (annual or monthly) from the mean of all longitudes, which he gives in a tabular form for every fifth degree of latitude and every tenth degree of longitude in the northern hemisphere, by means of interpolation from the observations discussed in Part I. From these tables it appears that in addition to, and superimposed upon, the general system of two polar cyclones due to the normal differences of temperature between the Equator and the Poles, we have throughout the year, and more especially in the winter, the conditions for the existence of a large fixed warm-centred cyclone in the North Atlantic, with its centre near Iceland. The barometric pressure should consequently be lower here than the mean of the latitude taken round the globe. That this is the case is well-known, and also that the prevalence of south-west winds in these islands is due to our generally lying on the south-east edge of this nearly perpetual cyclone. A similar cyclone similarly produced lies in the North Pacific.

Two corresponding regions of abnormally low temperature lie one on the east side of Asia, and the other on the east side of America, which, according to the author's theory, should give rise to cyclones with cold centres. As a matter of fact, however, these conditions are found to be completely reversed; the pressure being above the average, especially in winter, when the temperature-gradients are steeper, and therefore, according to the author's views, the cyclonic conditions should be more developed; while the motion of the air at the surface is anticyclonic, and outwards from the region of greatest relative cold.

The least satisfactory part of the author's work is that which relates to these cyclones with cold centres. Their non-existence in the progressive form is admitted, and where they should occur according to theory in a stationary form, they are notably absent, except in the two circumpolar cyclones. It is possible, however, that they may be identified, though in a modified form, and lacking the central barometric depression *at the earth's surface*, with what are termed "winter anticyclones," which usually coincide with areas of great cold, and which, while they exhibit at the earth's surface an anticyclonic outflow of air, are fed above a certain level by a cyclonic inflow.

Finally, as regards rainfall, which is an almost unfailing accompaniment of cyclones, the author, while admitting its assistance in helping to maintain a cyclone when once started, by the forces which operate whenever vapour is condensed, is strongly opposed to its being a *primary* source of energy, and cites in favour of this notion the following conclusion, arrived at by Prof. Loomis, after a careful study of the U.S. Signal Service charts. "Rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation or of their progressive motion."

The last chapter of the author's work which relates to tornadoes, waterspouts, and hailstorms, has already been referred to in a special article in NATURE, and it only remains for us to observe in connection therewith, that while tornadoes differ specifically in many respects from cyclones, the condition of the atmosphere in the latter is eminently favourable to their production. To this circumstance, according to Ferrel, may be attributed the occurrence of sudden blasts of tornado violence in the middle of cyclones, accompanied by a rapid oscillation of the wind-vane. It is these sudden gusts which do the main damage in such cases, since, as might be expected, the velocity of the wind increases *per saltum* where the gyrations of the tornado and the cyclone coincide in direction. They are found to occur more on the cold or clearing-up side of a cyclone, which Ferrel explains to be due to the cold upper strata overlapping the warmer central part of the storm, and thus promoting a condition of vertical instability of equilibrium in which tornadoes are generated with facility. Viewing the work as a whole, Mr. Ferrel may be congratulated on having presented to the world a memoir of such luminous research as well as practical utility. When we compare it with the numerous other crude treatises and hypotheses evolved during the past half-century on the same subject, which have not only brought the science of meteorology into ridicule, but encumbered our libraries, we feel a deep sense of relief at finding the question dealt with by a mathematician of more than ordinary ability, and one who does not shrink from tackling the real difficulties of the subject. He has for some time been known by his writings on hydrodynamical questions of great importance, especially those applying to the general motions of the atmosphere. The present work will go far towards placing him in the very front rank of physical and theoretical meteorologists. The deductive method has been fairly applied throughout to the equations of motion, and its success will do much towards counteracting the too prevalent tendency at the present time to induct from every solitary phenomenon, or experiment, to some otherwise baseless hypothesis. If the author has not accounted for all the peculiarities of cyclones, he has at least shown that the views entertained by the leading meteorologists regarding their formation, characteristics, and general movements accord with their mechanical theory, and that the sources of energy ordinarily assumed to act, such as heat, gravitation, and terrestrial rotation, are sufficient, without having recourse to any wild hypothesis founded on some unknown function of electricity. The valuable practical hints and suggested modifications of existing rules will do much to avert disasters at sea, the main purpose, doubtless, for which the work was designed, while its thoroughness and comprehensive character will materially help to advance our knowledge of a meteor, which in one form or another comprises almost every condition of the atmosphere included under the term "weather." E. DOUGLAS ARCHIBALD

ON PHOTOGRAPHS OF THE SPECTRA OF THE NEBULA IN ORION[1]

FOR about eighteen months I have been giving attention to the nebula in Orion with two objects in view, first to ascertain whether any changes are taking place in that body by making a series of photographs to be compared in the future with a similar series; and second, to photograph the spectrum of the nebula in various parts so as to see whether any new lines could be found, and also whether the composition is uniform throughout.

As to the first of these objects I have recently suc-

[1] Read before the National Academy of Sciences, April, 1882, at Washington, U.S., by Henry Draper, M.D. Communicated by the author.